Non-invasive ventilation

We read with interest the recent paper published in Thorax by Plant et al which analysed predictors of the outcome of non-invasive ventilation (NIV) in patients with acute exacerbations of chronic obstructive pulmonary disease. Few data are available on this important question, and studies specifically designed to propose answers are either retrospective or have been performed in a small sample of patients. We recently published the results of a trial using a design similar to that of Plant et al. Our findings indicated that the greatest predictive power lies in variables reflecting the evolution of arterial blood gases (particularly pH) after a brief (1 hour) initial trial period of NIV. Our multivariate predictive model had an adequate power of discrimination, correctly classifying more than 95% of the patients with a sensitivity of 97% and a specificity of 90% in a subsequently analysed sample of new patients. Our model therefore performed quite differently from that of Plant et al which had poor discriminant value even though they studied a greater number of patients.

The explanation for the differences between our findings and those of Plant et al may arise from the fact that they used data from a previous study that had not been designed to predict the result of NIV. In fact, Plant et al did not analyse all possible variables. In particular, they did not look at those which we found to be of value in predicting outcome. In addition, they did not analyse all available data using a stepwise approach.

Finally, as the model they proposed was not validated in a subsequent study with a new sample of patients, the clinical usefulness of the predictors is poor, as noted by the authors.

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References

Decision analysis in NSCLC

The paper by Dowie and Wildman exploring the decision analysis for surgically high risk patients with stage Ia non-small cell lung cancer (NSCLC) was interesting. However, I have reservations about the figures used to inform the decision tree.

There has never been a randomised trial comparing surgery with radical radiotherapy for these patients and the authors have taken 5 year survival figures for quite different groups of patients. The surgical survival figures come from a very large series of patients of all ages and levels of fitness, while the radical therapy figures come from a relatively small retrospective series of patients who were considered unfit for (or refused) surgery. These patients have, for reasons of age and co-morbidity, a lower background probability of 5 year survival. The Cochrane Review on this topic identified 26 studies, including over 2000 patients. For patients with T1 tumours from six studies, 5 year survival ranged from 29% to 37%, but there was a high rate of intercurrent deaths (median 25%). So, in the model, either the 5 year survival probability for surgery needs to be adjusted down or that for radical radiotherapy adjusted up to allow for the different case mix.

Also, we have no evidence on the natural history of untreated stage Ia disease. The probability of 5 year survival without treatment of 1% that is used in the model may be unduly low because it is the figure generally quoted for patients with advanced disease at presentation treated palliatively.

Why are the utilities for survival 10 points lower after radical radiotherapy than after surgery? A patient who is alive at 5 years with a given level of disability should express the same utility whatever the original treatment. The utility after surgery could, in fact, be lower because some patients are troubled by post-thoracotomy chest wall pain.
Like Treasure, I have the feeling that this model was derived from a theoretical study of the literature without adequate discussion with clinicians. It is, however, a start. I would be happy to cooperate on a more clinically based further exploration of the topic.

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References

Authors’ reply
We are grateful for the specialist input of Dr Macbeth who uses his expertise to contribute to the refinement of the probabilities and utilities in the decision tree. It was clearly not our intention to provide a definitive tree, simply an illustrative one which would demonstrate the potential of decision trees in integrating the best evidence with individual patient preference in a coherent and transparent way. But we were able to use this tree to confirm that the surgical mortality threshold for an individual patient should be determined by their expected mortality (and health related quality of life) under the alternative options, rather than by considering the surgical consequences alone.

The study by Gauden et al was selected to provide a point estimate of the 5 year survival possible following radiotherapy (range of 5 year survival in the Cochrane review 29–37%, Gauden study 5 year survival 32%). The Cochrane review highlights the fact that survival following radiotherapy is influenced by patients’ performance status, presence or absence of weight loss, and tumour size. The tree provides a structure to allow this complexity to be explicitly taken into account. We see decision trees allowing experts in the various disciplines involved in cancer care to operationally in their assessment of the value of analysis based decision technologies, but it is reassuring to see that there is increasing recognition of their future potential.

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Thorax 2002 57: 919
doi: 10.1136/thorax.57.10.919